

Touching the void

Discussion on Massive migrations? The impact of recent aDNA studies on our view of 3rd millennium Europe, by Martin Furholt (European Journal of Archaeology, 2017)

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Hardly a single week seems to pass without the publication of another aDNA paper on Later European Prehistory, bringing its sense of repetition in terms of methods and, most worryingly, results and limited archaeological contextualisation. And yet, despite the magnitude of this data-cascade, archaeological reactions remain comparatively limited and subdued (but see Hofmann 2014, Vander Linden 2016). From this point of view, this important contribution by Martin Furholt, and the decision by the EJA editorial team to attach to it a full discussion, must be applauded. Obviously, the relevance of Furholt's piece reaches well beyond its mere existence and lies first and foremost in its intrinsic quality and the key points it raises.

Furholt rightly stresses the uncomfortable position of archaeologists regarding aDNA. On the one hand, here is a method which demonstrates exquisite technical refinements, but, on the other hand, its interpretations are plagued, at least to the archaeologist's trained eye, by an outdated framework which treats migrations as securely identified historical events to be mapped and documented, rather than explained and understood. Even if these papers are published in the highest-ranking journals, one is left with the uneasy feeling that, in archaeological terms, very little has actually been learnt. Being for the Early or the Late European Neolithic, the existence of migrating populations has indeed been long suspected, though, as any student of our discipline knows all too well, their exact role in explaining change in past human societies has been systematically minimised since the 1960s.

The point is simple. As processual archaeology raised from the ashes of the pyre it had set on migration, the topic became, until relatively recently, a no-go area for most archaeologists. Furholt might be able to invoke several key papers when advocating a more complex take on migration, the field remains in its infancy. Likewise, the excessive liberty taken by geneticists with archaeological cultures reflects, in a distorted way, the unease of our discipline with this core concept and, beyond, how poor we generally are at describing material variability, let alone interpreting it (see Shennan 2013). All in all, Furholt's outlook reflects the state of uncertainty where the discipline has been lingering for too long.

In many respects thus, we created a void waiting to be filled, which geneticists did on the back of undeniable technical prowess. This being said, it is obvious that geneticists do not engage that much with archaeology and archaeologists, the latter often appearing as passive sample providers rather than active intellectual contributors. At the core of the problem, lies the assumed identity between biological populations and archaeological cultures, both considered as expressions of coherent, self-bounded units, a fallacy denounced fifteen years ago when modern DNA was hailed as revolutionary (e.g. MacEachern 2000). Two simple points, also made by Furholt, demonstrate the negative impact of this simplistic and false assumption.

Firstly, aDNA papers often stem from a limited number of samples, from which any pattern is then generalised across the entire geographical and temporal extent of the corresponding archaeological culture. The nature of aDNA data partly overcomes this problem (Li and Durbin 2011) and further – hopefully more systematic – sampling will by definition improve the resolution. However, the limitations of this approach are obvious as the narrow range of samples does not allow to explore possible differences within the geographical and temporal lapse of a given archaeological culture. Secondly, the question of relatedness between populations is central to the genetic enterprise, and

reflected in admixture and PCA graphs which provide statistically-informed depictions of this measure. The reasons for this preference lie deeply in the project of describing and understanding the variation of the modern Europe genetic variation. The difficulty lies when one attempts to translate biological relatedness in social terms, materialised by archaeological artefacts. This tension is well exemplified when genetic relations between central and eastern European 3rd mill. BC samples are linked to corresponding material affinities between the Yamnaya and Corded Ware complexes whilst, as Furholt elegantly reminds us, the latter is pretty much typologically-related to all archaeological cultures before, after and around it.

The above remarks arguably stem from a genuine lack of archaeological sophistication in most aDNA papers, in many ways repeating and prolonging the mistakes made by archaeologists for several decades. Are we thus in a theoretical impasse? Or is it that aDNA can only provide information we cannot do much with? Most surely not. The bulk of the work has so far been carried out in a deceptively empirical way, taking advantage of a so-called golden phase, where each sample is bound to tell us something that we by definition did not know about the genetic make-up of past populations. This strategy, whilst efficient in the short term, is hardly sustainable. The only viable alternative lies in complex *hypotheses*, informed and tested by several categories of evidence, all considered on equal footings.

Population history, including migration, provides a robust framework for such hypotheses to be elaborated. Indeed, the main result gained from aDNA, as well as from other studies, is that Neolithic populations were in constant flux, as suggested by ever-changing levels of admixture. This simple observation opens countless possibilities for future research. For instance, reading the literature, one can be left with the impression that the Early and Late Neolithic migration events were interchangeable, whilst they obviously happened under different demographic regimes, under different logistics, i.e. in fundamentally different ways. Were small or large groups moving? Did the migration involve all categories of people, or were they sex-biased? Both questions can partly be answered by bioinformatic treatments of the data (e.g. Pemberton et al. 2012, Goldberg et al. 2017). Further fine-grained structure in these migrations will without doubt be unravelled by re-examining in parallel the aDNA and wider archaeological records. However, when doing so, and as hinted at by Furholt, we need to drive away from our implicit will to align neatly all signals to write simple, elegant yet inherently faulty narratives. Genes and material culture, in their own complexity, do not operate in the same spheres of action, nor do they unfold upon the same spatial and temporal scales. We need to take advantage of their respective complexity to test alternative hypotheses, and get towards a more textured representation of the past.

To conclude, aDNA is without any doubt a fantastic technique, whose amazing potential we only begin to grasp. It is however worth reminding that this potential only applies to a narrow part of the archaeological agenda (e.g. Kintigh et al. 2014), and will only be unfolded by testing proper hypotheses, rather than chasing ghosts of our discipline's past. Of all social sciences, archaeology has assuredly the best track record at inter-disciplinarity, and thus at overcoming a range of responses from extreme confidence to total disenchantment with new techniques. In this sense, the difficulties we encounter with aDNA are hardly new. Let us just not forget that any improvement will not simply come from “us” teaching “them”, but also from us giving a hard look at ourselves in the mirror.

References

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